

*MACHT'S NICHT? A COMMENTARY ON STADDON'S
"THE CONVENTIONAL WISDOM OF BEHAVIOR ANALYSIS"*

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Any commentator on Staddon versus Branch has a distinct advantage in that Branch's editorial and Staddon's response to it are necessarily superficial. Branch's was not a deep and scholarly treatment of philosophical and methodological foundations of behavior analysis. Rather, he sketched some general orientations guiding his particular views of the kinds of papers most appropriate for the *Journal of the Experimental Analysis of Behavior*. Staddon, in responding, found some apparently easy targets. However, the issues brought to light in Staddon's commentary are clearly worth a more careful treatment than either Branch or Staddon intended to provide.

Staddon's criticisms focus on environment-versus organism-based explanations, the role of Darwin and Mach as antecedents to the behavior-analytic approach, Newtonian mechanics as a model for behavioral analysis, and language, especially the necessity for a precise terminology. Anything like a thorough analysis of all these topics as they relate to behavior theory would require a book-length manuscript. In the present commentary, I would like to consider primarily Staddon's treatment of the contributions of Mach and those of Newtonian mechanics to behavior analysis.

Mach Machts Recht

Staddon's treatment of Mach truly reflects "conventional wisdom." Mach had a status even in his own day not unlike that of Skinner in his lifetime. Even those who knew Mach could greatly misrepresent his views, and others who wrote about him either misunderstood his writings or got their views from sources who had apparently never read him. The stereotypical Mach emerged—the rigidly inductive empiricist insisting that only directly sensed entities or facts belong in a scientific account,

and that a scientific theory should comprise only an arrangement of such facts in some economical order. No wonder Mach became the whipping boy of most who hold a more catholic view.

There is a scintilla of truth behind all stereotypes, and indeed, in his cantankerous and painful last days Mach appeared to reject much of what was to form modern physics—atoms and relativity. The reasons for rejecting relativity are unclear, especially because Einstein himself had been inspired by Mach's critical treatment of Newtonian "absolutism." The situation with atoms is more directly relevant to the issues at hand because it is the strongest illustration of Mach's reputed insistence on "observability." His actual views are far more complex and sophisticated. The treatment that follows is derived principally from Laudan (1981; see also Cohen & Seeger, 1970; Mach, 1960; Marr, 1985).

Laudan (1981) argues that Mach's rejection of atomism had little or nothing to do with his sensationist position or with the scientific evidence. Rather, his views were based primarily on general methodological grounds, that is, on criteria for useful scientific theories. First, Mach did *not* assert that only what was given by the senses be the elements of a scientific theory. Every theory includes entities or concepts not subject to sensory scrutiny—moment of inertia, potential energy, charge, and so forth. As Laudan puts it, "He has no general axe to grind against theorizing as such, nor against most of the scientific theories of his day, despite the fact that virtually all of them go well beyond what a sensationist account of knowledge would legitimate" (1981, p. 204).

Moreover, 19th century positivism actively supported theory construction as a means not only to describe present phenomena and predict new phenomena but also to frame coherent approaches to an experimental analysis. To quote Mach, "Without some preconceived opinion the experiment is impossible. . . . For how and on what could we experiment if we

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did not previously have some suspicion of what we were about?" (1960, p. 161). This perspective dissolves the myth that he proceeded along purely inductive lines, letting a theory emerge like Aphrodite from a sea of data. This is also supported by his admiration for men like Faraday, Carnot, and Darwin.

Mach did espouse a very conservative view (shared by most scientists of his day) about how theories should be constructed. A key feature was parsimony; theories should be economically constructed without unnecessary entities or processes. The aim was description and prediction. The proposed entities or processes should be, in principle, capable of direct verification. If not, then they could, at best, serve only as props or scaffolding until such time as they could be eliminated from the theory. The last point is especially crucial. Here is what he had to say about Franklin's notion that electricity was a kind of fluid:

The electric fluid is a thing of thought, a mental adjunct. [Such] implements of physical science [are] contrived for special purposes. They are discarded, cast aside, when the interconnection . . . has become familiar; for this last is the very gist of the affair. The implement is not of the same dignity, or reality . . . and must not be placed in the same category. (quoted by Laudan, 1981, p. 212)

For Mach, atoms were "things of thought."

The only value of such contrivances is *heuristic*. They may serve as guides to the development of an analytic account. Once that task is accomplished, however, the implements are no longer necessary. A beautiful example is classical electromagnetic theory. Faraday embraced the notion of "lines of force" in his theoretical and experimental analysis of electromagnetism. (He actually came to believe in them.) When Maxwell completed his mathematical account that encompassed the principal phenomena of electromagnetism, lines of force were no longer of any relevance. Measurable properties of electromagnetism in a given situation were predictable from solutions of Maxwell's equations.

Despite the heuristic value of contrivances, Mach warned of the dangers of confusing a mental artifice with the actual phenomenon of interest. The artifice itself becomes the object of investigation, or worse, the explanation. Our fancies become facts. Faraday in 1844 was aware of the problem:

The word *atom*, which can never be used without involving much that is purely hypothetical, is often intended to be used to express a simple fact; but good as the intention is, I have not yet found a mind that did habitually separate it from its accompanying temptations [i.e., the temptation to think of atoms as real]. (quoted by Laudan, 1981, p. 220)

And thus, he himself was seduced by his own lines of force.

Enough has been written about how behavior analysis fits into a generally Machist framework (see, e.g., Marr, 1985, for a brief summary). But I would assert further that, in general, modern science, in all its forms and substance, has continued to employ as an ideal much of Mach's *Wissenschaftstheorie*, unacknowledged, of course, if not outright denied.

Newton and the Arrow of Time

Staddon raises some stimulating issues regarding Newtonian physics as a model for behavior analysis. Certainly, the Newtonian legacy embodied in classical mechanics is a deep and rich source for models of both behavioral statics and dynamics (see, e.g., Killeen, 1992; Marr, 1992; Nevin, 1992). Indeed, Staddon is a master at generating quantitative models of behavior, most of which could be represented within a mechanistic framework.

Newton is the apotheosis of order, predictability, and parsimony; no other theory of natural phenomena encompasses so much with so few principles. As behavior analysts, we could do worse than to aspire to that kind of model. I do not believe Branch was saying much more than that. However, Staddon is correct in pointing out the temporal symmetry in Newtonian physics. Irreversibility, however, is inherent in behavior change, as it is with most phenomena around us. In emphasizing this, Staddon touches upon some of the deepest, most complex, and controversial issues in modern scientific history (see, e.g., Coveney & Highfield, 1990; Davies, 1974; Gardner, 1990; Nicolis & Prigogine, 1989). Of special concern in the context of behavior-analytic theory is how to bring the irreversible history of an organism into account.

How should we characterize a "history"? Staddon provides a helpful perspective: Given different histories, different effects may ensue from the same initial conditions. Models characterizing behavior change do not necessarily

have to invoke a history. In that sense, they may be described as "Newtonian." In general, any model whose structure comprises some simple function of differences between "states" (specified only on the basis of initial and final conditions) would fall into the Newtonian category. Newtonian models may be quite successful within certain domains, namely those maintained close to equilibrium (i.e., steady state), with small or very slow changes in conditions.

Behavioral systems are, in general, nonlinear, dissipative, and far from equilibrium, and thus may exhibit a history. The key problem, as presented by Staddon, is that the effects of history may be silent until some intervention occurs; that is, initial conditions, *traditionally interpreted*, are not enough for prediction. Of course, we might be using an inadequate set of conditions, not only in the measures we use (rate, etc.) but in their distribution functions as well. Extending the meaning of initial conditions to encompass measures extended in time, as Staddon points out, reinserts the approach into a Newtonian frame. Nevertheless, time clearly plays a different role in behavior dynamics than it does in classical Newtonian dynamics.

Until recently, behavior-analytic theory, no matter how quantitative in approach, was concerned with description and prediction of steady-state or equilibrium performance. However, as the recent special issue of the *Journal of the Experimental Analysis of Behavior* (May, 1992) demonstrates, dynamic models of behavior change are receiving accelerated attention. Few, if any, of these models could reasonably be considered organism based, in the sense that assumptions comprise specific events going on inside the organism. The nearest approximations to this kind of model are those that assume that the effects of reinforcement decay over time. Staddon presents such a model and argues that in these kinds of models distinctions between environment- versus organism-based accounts are simply a matter of perspective, and the two accounts are equally valid and useful. The differences of perspective hinge on how one interprets "effects." For the behavior analyst, effects are embodied in possible functional relations between measures of behavior and the temporal properties of reinforcement. Such relations are analogous to Newton's law of gravitation, except that that

law invokes a spatial, as opposed to a temporal, influence. The law, as with other laws, does not force or imply any particular mechanism underlying the relation. The great advantage of an analytic account is that it does not depend on any particular picture for its application. Why invoke the notion of a "trace" within the organism when all one wants to achieve is an expression of action at a temporal distance? The functional relations include the events of interest; they do not depict a dying echo in the brain. Moreover, invoking a trace as the primary event that decays tells us nothing whatever about how trace becomes behavior.

The argument for emphasizing environmental as opposed to organism-based theory is not some kind of antiphysiological stance. There is, however, a strong bias against a fantasy physiology with properties just needed to deal with the problem at hand. It is also important to emphasize that this has little or nothing to do with "observability." The problem with traces is *not* in their unobservability, but that they may be unnecessary, or worse, misleading fabrications. Staddon's cumulative effects model, by his own account, in no way requires any pseudophysiological mechanism. "History" is embodied in the analytic procedure, including some assumed function that defines changes in reinforcer (or response) effectiveness over time. The "state" of the organism is simply the sum of its histories in the previous sessions, but the referents for this state, other than the values obtained by the calculation, are totally unknown, if not irrelevant. The unit of V is reinforcer divided by response; what is organism based about that? Why talk about states? The calculation is unambiguous; the state is not.

Of course, we will gradually begin to understand physiological mechanisms in behavior change. As that work progresses, accounts will increasingly involve interactions between environmental histories and physiological mechanisms.

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